First response to comments by Prof. Colin Prentice

First of all, I would like to thank Prof. Prentice for his lengthy and detailed review. In my response to his comments I would like to take the opportunity to explain more clearly the reasoning behind the development of the climate constrained vegetation index (CCVI) model.

Process model

The reviewer appears to like the general idea to build a relatively simple model that can simulate vegetation index data from environmental data, but finds the approach chosen less suitable. In particular, the reviewer would have liked a process based approach in which PAR would have been included.

My aim was to build a model that predicts NDVI (with the ability to estimate from this associated parameters such as LAI, fAPAR etc) that can be incorporated into land-surface models and ecological models without a need for their redesign. Therefore, I avoided incorporation of extensive hydrological and/or carbon modelling (which require calculation of evapotranspiration, representation of soil depth or plant water availability, photosynthesis, use of plant functional types, leaf parameters, carbon allocation to leaves etc) since such an implementation is likely different from those in existing land surface or ecosystem models. The Lieth model is one of the simplest models around, it only requires temperature and precipitation without modification, and despite or thanks to its simplicity, the NPP it calculates reproduces a proportion of the spatial and inter annual variability observed in NDVI; it therefore seemed a model worth considering. It did need adaptation to a monthly time step and also needed to be adapted to different climate regimes. The model also needed to predict NDVI, rather than NPP, and this is an important distinction to make. For example during wintertime in large areas of the boreal forests NDVI is above zero (usually in the range between 0.1 and 0.3) and NPP is zero. This NDVI between 0.1—0.3, is linked to a lower albedo and hence higher absorption of solar radiation, which results in higher winter temperatures. This is of importance to get a realistic energy budget in climate models. Thus there are cases where NDVI is not directly linked to NPP, and where this difference matters.

The simplicity of the CCVI model, it depends on temperature and precipitation only, allows it to be incorporated into other, process based models, without major modification or adaptation. If one so wishes modifications can of course be made, e.g. hydrological fluxes can be incorporated to improve estimates of water availability to plants. A recent paper (Los 2015) indicates that this type of modification does not always lead to an improvement and that use of unmodified precipitation data may lead to better results, but this is an aside. Thus the purpose of the study was to build a simple sub-model that could easily be incorporated in more complex process models (land surface or ecosystem models), this model should produce output that could be used in lieu of the satellite based NDVI. Models that depend on satellite derived parameters can thus be used for simulations of the past century as well as for simulations of past and future climates. The objective is not to replace process models, but to fix one particular problem in at least a proportion of them and overcome their inability to simulate realistic spatial, seasonal and inter annual variability in leaf area, fAPAR or NDVI.

PAR

I did consider adding PAR to the model but decided against it based on the following considerations:

1. When looking at photosynthesis models (Farquhar, Berry, Collatz, … see Fig. 1.1) one sees that photosynthesis increasing linearly with PAR at low light levels but reaches saturation quite quickly. The saturation point changes with temperature, for low temperatures saturation is reached very quickly, for high temperatures saturation occurs at higher light levels. Thus at the leaf level we find that radiation is only limiting at low light levels and we also find an important effect in the light limitations set by temperature. At canopy level the light saturation disappears (e.g. Alton et al 2007), but there are also indications that solar radiation, in many cases, is not a limiting factor (see e.g. partial correlations below). This is not to say that PAR is not important, it does suggest that temperature as a limiting factor on leaf growth (not NPP) may have merit not only because of its relationship with PAR but also in its own right.

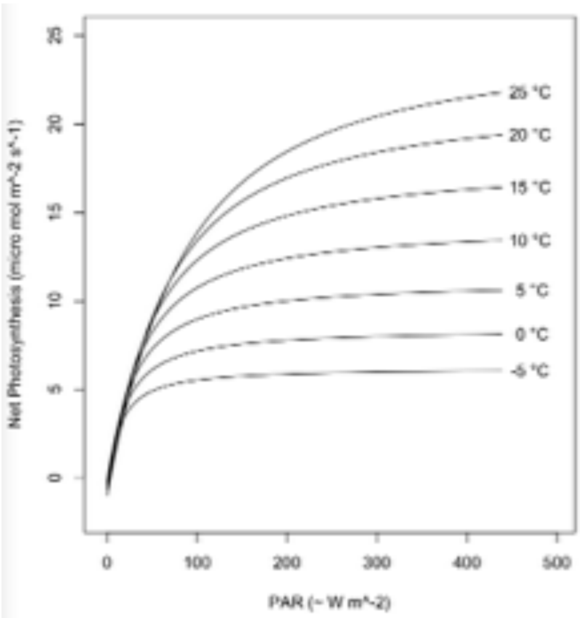

Fig. response 1.1: Photosynthetic rate as a function of PAR and temperature predicted by the Farquhar model, other parameters not varied.

2. The importance of PAR was explored in more detail in the RVI paper (Los 2013) where NDVI is simulated from monthly temperature and precipitation data using locally optimised regressions. Residual variance in the observations, not explained by the RVI, was compared with, amongst others, shortwave radiation. There were many significant correlations between the residuals (observed - modelled NDVI) and solar radiation, but the magnitude of the explained signals was small and their sign varied from location to location. Thus I found little evidence that PAR would help improve the (RVI) model. To test the reviewer's comments further I add below Fig 1.2 which shows the partial correlations between mean annual NDVI (1983-2000) and mean annual NDVI of the preceding year (see below), mean annual temperature (1983—2000), precipitation (1983—2000) and cloudiness (1983—2000). The highest correlations are found for temperature, then precipitation and NDVI of the previous year and finally cloudiness (see

also table below). When looking at correlation split out by KT class the partial correlations between annual T and NDVI are much higher than for cloudiness and NDVI in high latitudes (class 5 and 6 in Table 2). Thus contrary to the reviewers comments it appears that annual temperature is a much more important explanatory variable for annual NDVI than annual cloudiness or PAR.

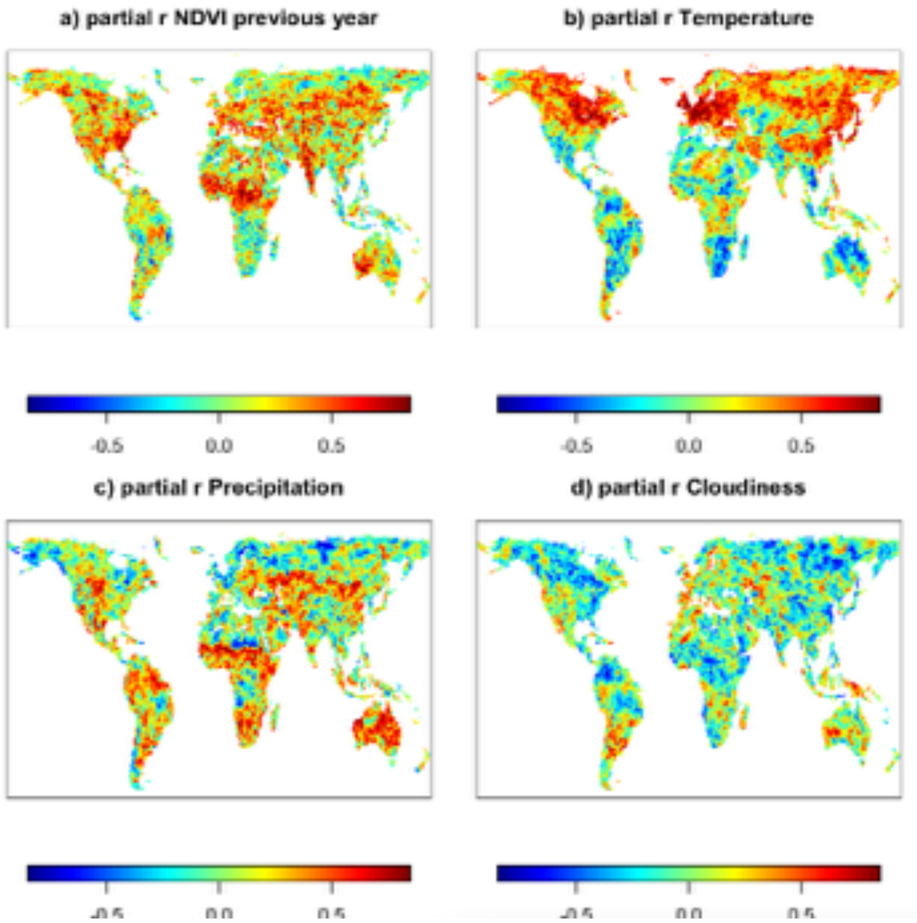

Fig. response 1.2: Partial correlations of mean annual NDVI (1983-2000) with NDVI of the previous year (see section low interannual correlations), annual temperature, precipitation and cloudiness (latter three are CRU v3.21 data).

Table 1: mean partial correlation coefficient r, variance explained and number of cells significant correlations at $p < 0.1$ of annual NDVI time series 1983—2000 with annual NDVI 1982—1999, annual temperature 1983—2000, annual precipitation 1983—2000 and annual cloudiness 1983—2000.

	mean partial r	mean partial r^2	n cells p < 0.1
NDVI(year - 1)	0.184	0.115	14164
temperature	0.163	0.14	18296
precipitation	0.123	0.118	14944
cloudiness	-0.049	0.083	9153

Table 2: same correlations as table 1, but per KT class

	r (NDVI - 1)	r (T)	r(PPN)	r(Cloud)
KT1	0.13	0.003	0.16	-0.05
KT2	0.19	-0.03	0.21	-0.01
KT3	0.17	0.05	0.12	0.05
KT4	0.27	0.33	0.16	-0.01
KT5	0.14	0.35	-0.04	-0.14
KT6	0.09	0.32	-0.02	-0.14

3. **Monthly time step**: The CCVI and RVI model NDVI at a monthly time step, whereas the papers by Bonan (1993) and Zaks et al (2007) look at annual NPP, as does the Lieth Miami model and other models tested by Zaks et al (2007). A key question is if monthly, rather than annual PAR, is suitable for the CCVI model. It is well known that in temperate latitudes, where in the Lieth formulation temperature is limiting, the seasonal cycle in solar radiation precedes the temperature cycle, e.g. minimum and maximum solar radiation occur during December and June, whereas minimum and maximum temperatures occur 1-2 months later (Fig 1.2). The timing of minimum and maximum NDVI tend to coincide with the occurrence of minimum and maximum temperature, not with minima and maxima in the seasonal solar cycle. This is illustrated in the figure below (Fig 1.3) which shows the difference in timing of the peak in radiation and NDVI and the same for temperature and NDVI: Thus in temperate NH latitudes using the PAR as a limiting factor would likely lead to errors in simulating the seasonality of NDVI, in particular in timing of minima and maxima. Some models have been criticised for producing the wrong lag in the vegetation seasonal cycle (e.g. by Randerson et al 2009) and this was a criticism I would like to avoid. Another consideration not to use PAR was that some areas have low temperature and high solar radiation, and in these areas one would predict high NDVI, given sufficient precipitation (e.g. Himalayas). Zaks et al (2007) solve this problem by using a temperature threshold for the PAR based model, but I find this approach less elegant. If, for example, PAR were limiting in the CCVI model, CCVI would increase with height in mountains (given sufficient precipitation) whereas it now decreases with height because of temperature limitations.

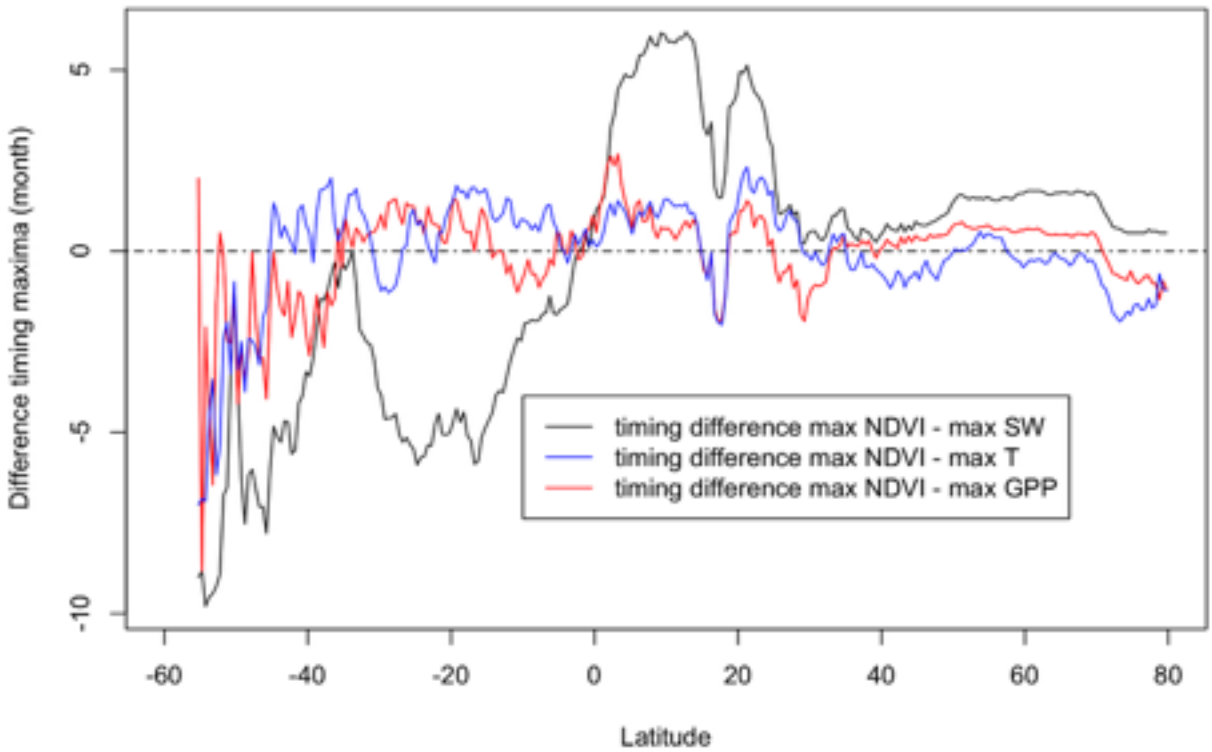

Fig. response 1.3: Difference in timing of month with maximum NDVI averaged by latitude for 1982—1999 compared to timing of maximum in 3 other parameters: SW radiation (NCEP reanalysis 1), temperature (CRU) and GPP (Jung et al 2011). For NH temperate latitudes time of peak of NDVI and temperature closely agree, radiation leads NDVI by about 1.5 months. The timing of max GPP in temperature NH latitudes is between the timing of max solar radiation and max temperature or max NDVI.

4. As a not unimportant aside, the analysis by Zaks et al 2007 shows that Lieth's Miami model actually performs better than the other 3 models they investigated when looking only at the coefficient of correlation and the RMSE (See their table 3); although differences between the models they tested were small. They also looked at latitudinal plots and then find that the Lieth model does not predict local maxima in NPP at latitudes of 40 S and 50 N, but the PAR based model does predict these to some extent (see their Fig. 6). The use of a monthly time step and derivation of climate zone dependent functions to derive CCVI in the present study go a long way in alleviating this latter problem. I include a plot below (Fig. 1.4) where observed latitudinal average in NDVI are compared with modelled latitudinal averages in CCVI.
5. The abstract by Monteith (1977) reads: "… Crop growth in Britain may therefore be analysed in terms of (*a*) the amount of light intercepted during the growing season and (*b*) the efficiency with which intercepted light is used. The amount intercepted depends on the seasonal distribution of leaf area, which in turn, depends on temperature and soil water supply. …". The CCVI follows this approach, the CCVI is calculated from precipitation and temperature and the intention was to use it with PAR to estimate NPP.

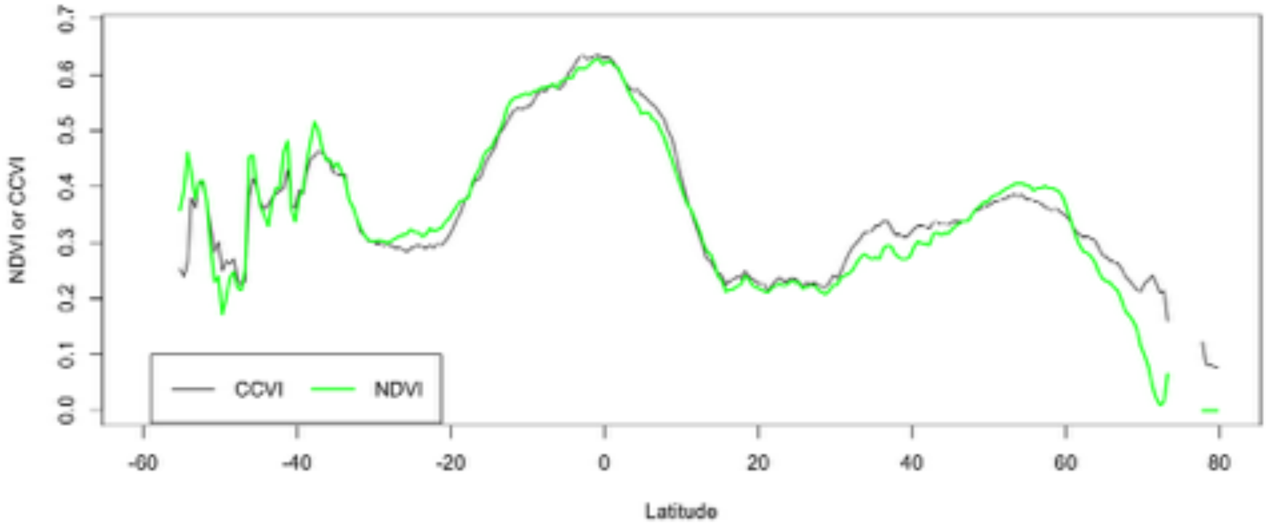

Fig. response 1.4: Mean latitudinal average NDVI and CCVI; CCVI clearly shows 3 vegetation maxima around 40 S, 0 and 50 N that did not occur in the original Lieth model according to Zaks et al 2007.

6. Another consideration in particular for the simulation of CCVI for the 20th century was that global coverage and quality of temperature data and precipitation data is much better than that of PAR data. I agree with the reviewer that PAR can be estimated from cloudiness and that cloudiness data are available for the 20th century, however these cloudiness data (e.g. the CRU data) are based on fewer observations than the temperature and precipitation data and therefore rely more heavily on climatologies to fill gaps in the record and as a result do not show the same amount of inter annual variability as temperature or precipitation data. Moreover, cloud cover is usually estimated by an observer on a scale from 0-8, rather then directly measured; hence it is a more subjective, cruder measure than temperature. Using PAR in stead of temperature would give less opportunity to test the model on past data because a large proportion of inter annual variability is either not captured in the cloud data or is of lesser quality.

Other issues

There are several other issues raised by the reviewer, which reveal places where the text needs further explanation:

the use of SR to estimate CO2 fertilisation. The formulation of the Lieth model by Esser et al uses a multiplication factor to incorporate CO2 fertilisation, in principle I could have used something analogous like:

NDVI(CO2) = NDVI(at standard CO2)*f(CO2)

with NDVI(CO2) the NDVI under current atmospheric CO2 conditions, f(CO2) the CO2 fertilisation factor and NDVI(at standard CO2) the mean NDVI for the period of 1982—1999. To estimate f(CO2) one needs to divide NDVI(CO2) by NDVI(at standard CO2) and this is problematic for areas where NDVI is zero or close to zero. By converting NDVI to simple ratio (SR = (1 + NDVI) / (1 - NDVI)), NDVI=0 will become SR=1; division by 1 is not a problem and therefore the f(CO2) can be estimated. After estimation of f(CO2), the SR can be adjusted for atmospheric CO2 concentrations and converted back to NDVI. The linear relationship between f(CO2) and SR was a coincidence.

Low inter annual correlations:

The comment in response to presenting low inter annual correlations between model and NDVI… "the fit of inter annual variability is poor by any standard "… needs to be seen in context of residual errors in the NDVI data and the % of land surface where inter annual variability is small (deserts and tropical forests). The figure below (Fig 1.5) shows the spatial distribution of the magnitude of the monthly anomaly in NDVI expressed as a standard deviation; it also shows the frequency distribution of the standard deviation. Overall the mean standard deviation of the anomalies is around 0.035 NDVI. This number needs to be compared with average residual errors in the NDVI associated with sensor calibration (0.001 for high NDVI, 0.002 for low NDVI), residual BRDF effects (0.02 and 0.001), residual cloud effects (0.005 for both), and residual volcanic aerosol effects (0.02 and 0.005 for two 2-year periods). The magnitude of the combined residual errors varies from location to location, but is estimated to be around 0.01 for low NDVI values and around 0.02 for high NDVI values. Thus the maximum variance that can be explained in the NDVI anomalies is around 60 % to 40 % (max r approximately between 0.75 and 0.65). This is assuming no errors occur in the climate data, if these are taken into account the maximum possible r is expected to drop even further.

For some areas (deserts, tropical forests), during some periods (winter / dry season) inter annual variability is low, for these times and places the correlation is expected to be around zero.

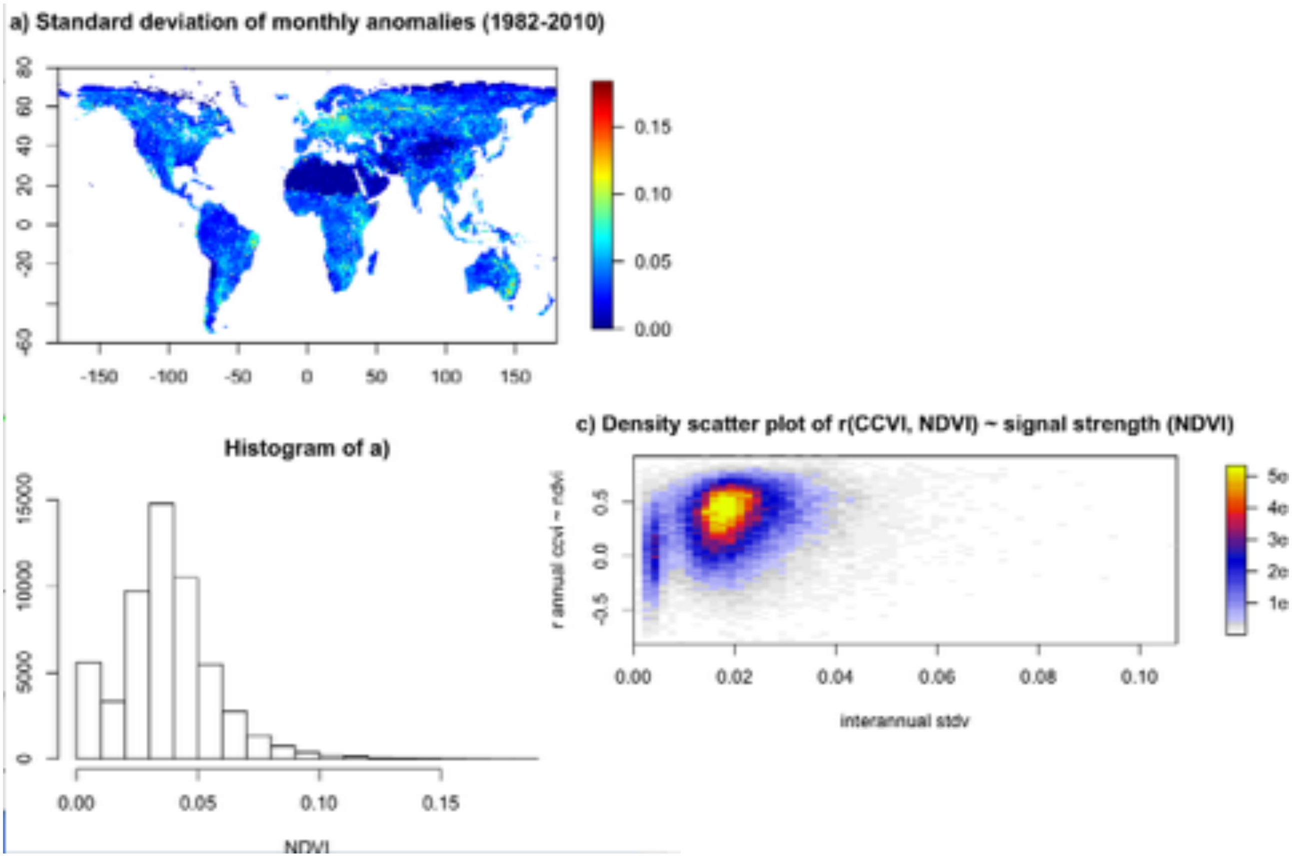

Fig response 1.5: a) spatial distribution of the magnitude of the anomaly signal in NDVI expressed as a standard deviation. b) Histogram of a. Median magnitude of anomaly signal is around 0.035 NDVI. c) density scatter plot showing correlation between NDVI and CCVI as a function of the magnitude of the signal in NDVI

The suggestion to incorporate a term to carry-over non-structural carbohydrate reserves from one year to the next to improve the inter annual variability is worth exploring further (see also table 1 and 2). The RVI does include a term that carries over to the next year (max NDVI of the past year) that could be linked to carbohydrate reserves (as well as seed formation). The CCVI does not include such a term, and I would like to explore if such a term would improve modelling results, especially since the partial correlation analysis (Fig 1.2, Table 1 and 2) suggests that this may be an important factor.

Segmented regression
From the description by the reviewer I would guess breakpoint regression and segmented regression are different names for the same method. With segmented regression the number of breakpoints and a first guess of their location is provided to the algorithm by the user, after which the algorithm estimates the "best" breakpoints using random variations. Variations in the locations may occur dependent on starting values and random numbers selected by the algorithm. Lowess has a degree of arbitrariness just like segmented regression since the stiffness of the smoothing (and other parameters) need to be set by the user as well. In addition, Lowess code is more difficult to port outside the statistical environment e.g. to land surface models or ecosystem models than the output of segmented regression and it will be more difficult to extrapolate lowess results to temperature and precipitation measurements not encountered when the model was developed. A similar argument can be made for the portability of GAMS (which in addition requires starting values and a range of decisions made by the user as well to obtain a solution). In the end I would expect to obtain similar results with any of these methods. The segmented regression has advantages in its portability, and it is conservative when it comes to inflation of errors.

Interannual variability Australia
I will add a figure showing time series for Australia similar to the figure for the North American and Sahel droughts. Below is an example with the top figure showing the average FASIR NDVI (green) and CCVI (black, dashed) time series for 1982—2010 for Australia / New Zealand and the bottom figure showing the CCVI time series for 1901—2010, indicating wet periods during the 1970s, and around 2000. The increase in CCVI for 2009-2010, a wet period, is larger in the CCVI than in the NDVI.

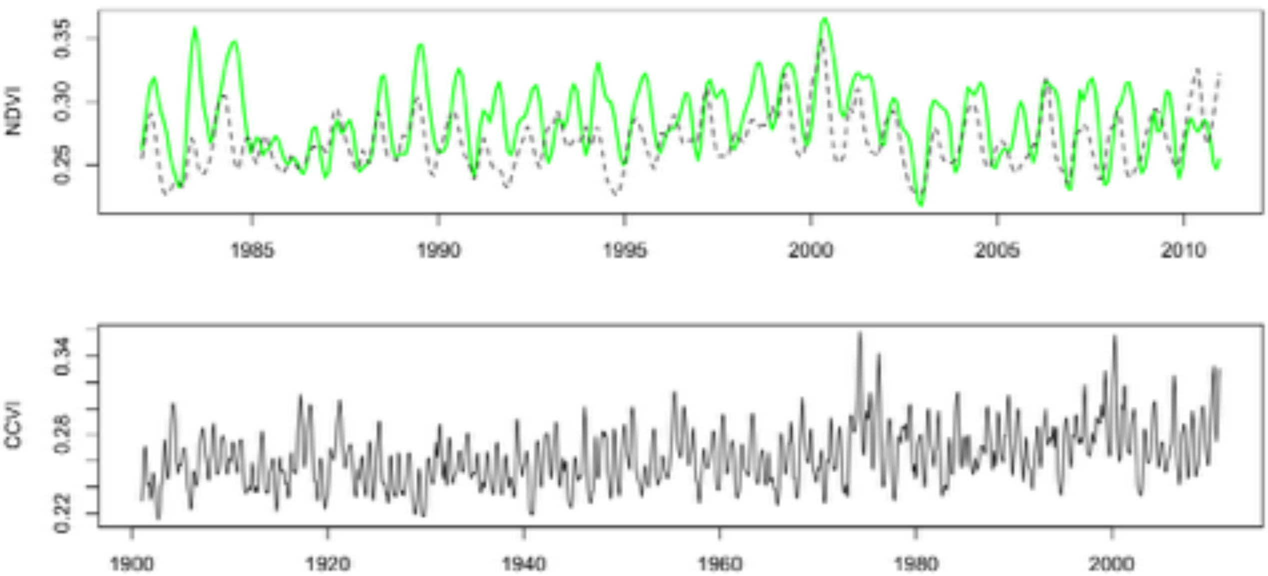

Fig.response 1.6: top) mean NDVI (green) and CCVI time series for Australia and New Zealand. NDVI is suspect for the latter half of 1994. CCVI captures various drought and wet events for Australia; b) CCVI for 1901—2010 indicating increased NDVI for the 1970s and around 2000 and 2010.

C**omments on writing / layout etc**:

I will adjust Fig. 1 to make it less ambiguous (I use 2 x-axes one on top and one at the bottom) and will add explanation to Fig. 11.

The suggestions to expand the discussion of model evaluations (Kelley et al 2013), sensitivity of inter annual variability to precipitation (include work by Piao et al 2013 and Beer et al 2010) and compare estimates of CO2 fertilisation for warm dry areas in Australia (Donohue et al 2013) are excellent and I will be happy to incorporate these.

Summary

To summarise, I developed a model that produces more realistic spatial, seasonal and inter annual variability than other models. I do think there is room for incorporating more effects (previous year NDVI, C3/C4 effects as suggested by reviewer, but also N fertilisation and agriculture). For the revision I would like to look at incorporating NDVI of the previous year and will test the effect of PAR more thoroughly, although I am doubtful its incorporation will make a large improvement. Other effects, such as the dependency on C3/C4 plants and crops versus natural vegetation, I would like to explore in future papers. I would like to state more clearly hypotheses, expand discussions in my paper and provide better explanations for choices I made. As it stands the CCVI model is a significant improvement over current practice and as such should be of interest to the community.

Bibliography

Alton et al. 2007, Global Change Biology 13: 776–787. Beer, C. et al. (2010) Science 329: 834-838. Bonan, G.B. (1993) Tellus 45B: 397-408. Donohue, R.J. et al. (2013) Geophysical Research Letters 40: 1-5. Kelley, D.I. et al. (2013) Biogeosciences 10: 3313-3340. Monteith, J. L. 1977, Phil Trans R Soc Lond B 281: 277-294. Piao, S. et al. (2013) Global Change Biology 19: 2117-2132. Poulter, B. et al. (2014) Nature 509: 600-603. Zaks et al. (2007) Global Biogeochemical Cycles 21 GB3004.